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ABSTRACT

A historical and anthropological analysis of the nature of conventions held by the American Anthropological Association (AAA) is presented. The author, who is an anthropologist, plays the role of participant observer in studying the phenomenon of meetings of the AAA. During the late 1940s and early 1950s, AAA meetings were very small and scholarly. Following World War II there was a movement away from pure scholarship toward more practical applications of anthropology, probably in order to make the AAA more relevant to research being conducted and funded by the government. Social structure within the AAA became pronounced throughout the 1960s as the association membership increased and regional societies held their own meetings. In response to fluctuations in the economy and the job market, these meetings varied in the extent to which parties were given by member universities to recruit graduate students. Age- and status-conscious behavior became apparent as older, better-known scholars sought out their colleagues at the meetings and young students could only make acquaintance with other young students who were attending. Little intellectual interaction took place because the older scholars generally attended to present papers, not to attend sessions given by others. Young students attended mainly to hear the papers, not to present them. Visible stratification supported the differences among attendees in the form of color-coded name tags to reflect member, fellow, or student status. The author concludes that the AAA is just as trapped in cultural constraints as are the societies which its members study. (AV)

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ANTHROPOLOGISTS IN THE LOOKING GLASS

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Participate. Observe. Learn. Better yet, know. And the best way is to start from the known and hope to unveil the unknown. Besides, if you are a newcomer in a group, the best thing is to watch the natives so that - given a long enough time - you too can melt into the woodwork along with the best adapted. In any event, a habit that begins from necessity (or how else can one discover what the score is at your very first meeting anyhow?) sometimes continues, in various guises, for a lifetime. In fact, if the "game" is kept up long enough you may find that you are learning things that you'd rather not know, or even that interpretations that you made years ago don't seem to fit quite as well as you once thought.

I'd like to start today by sharing with you a number of points of view that have affected my thinking about this group called Anthropologists. This will be followed by a summary of some hard data, after which I'll return to discuss some of the hazards of self-study as well as a suggested reinterpretation of the "conclusions". The journey is a personal one, but one which we are all in some state of experiencing - whether or not we come to agree on the context in which we place these experiences.

Some years ago, in a symposium on training for field work presented at a National Meeting in Chicago (in the early 50's, if I remember correctly) Margaret Mead assured us that if one wished to study and understand the religious experiences of others one had to have had such experiences oneself. To have heard this literally might have suggested that only mothers could study mothering, only corporate businessmen had the "insider's touch" to get at the intricacies of their society, Indians should study Indians, blacks blacks, etc. Obviously, this is extrapolation beyond Dr. Mead's intention, but my interpretation of a phrase dropped, perhaps in passing, so many years ago indicates how new contexts can be created in looking again at data collected over a long period to time. Thus, from Mead's comment one gets to: if you think you know what the group you believe in "stands for" you are in a "better" position than the outsider to study yourself - so here we are, anthropologists studying our own shadows the better to understand ourselves.

At about the same time an article appeared in what used to be the Southwestern Journal of Anthropology dealing with the Replicate Social Structure. What has stuck with me over the years from this paper has been the idea that if you know one part of the social system well enough, you will know all parts since other segments of the society simply provide other stages on which to play out the same central concerns. I might note in passing that a full commitment to such a viewpoint wreaks havoc with the central pillar of the anthropological mode of work - namely, that we are holistic. If the part can substitute for the whole, why knock yourself out being redundant?

And just the other day I learned of Kenneth Burke's approach in literary criticism which deals with change in literature, an approach which has been applied to an understanding of stylistic change in art and which, by extrapolation, may well serve as an alternate to the Kuhn model of paradigm shift in understanding changes. This approach states that change comes from a conscious misreading of the evidence which makes it possible to see therein what was earlier not clearly evident. If such "new readings" are possible, we must conclude that the groundplan doesn't change, although our interpretation does. In this sense "reality" floats with no fixed points, since reality shifts with each new interpretation. The data don't change, only the "sense" we make of them, but it is that which makes all the difference. Perhaps it is here that we may get to the "art" of anthropology, as something separate from the scientific replication of not too surprising data.

We'll return to these points later. Now for the "hard facts".

Two weeks after Pearl Harbor the last of the old style annual meetings of the AAA took place at Phillips Academy in Andover, Massachusetts. There was a single program and people didn't stand out in the hallway much at that time (they would have been missed). Everyone ate together in the Great Hall and two bug-eyed undergraduates drank it all in, amazed at being in the presence of all those "real greats" (and some not so-greats) and, in general, behaved like neophytes whenever their first meeting might be. The paper sessions were plagued by the same problems we still face: the Chairpersons were too polite to shut people up as they droned on and on (45 minutes on the manufacture of eoliths in a concrete mixer, for one), discussants insisted on pointing out that when THEY were among the Bingo Bongo the mother's brothers all spat out of the left side of

their mouth, while other discussants insisted that in "their society" it was otherwise. Though there was clearly a social structure to this meeting, we were too busy establishing the baseline to make many profound observations. One thing, however, was clear. The anthropologists present were certain they had something very substantial to contribute to the war effort and they were going to be certain that their expertise was appreciated and utilized. This was a patriotic duty, and it was evident that some in the group were reasonably sophisticated in knowing how to integrate themselves into the rapidly established bureaucracy so that, in fact, major anthropological input was effected. It was, at that time, considered patriotic to serve the country in the area of one's expertise (even if this only related to how school age youngsters responded to being fed lukewarm meals - just in case mass feeding would ever be necessary).

Meeting-going expanded over the years to include not only the annual corroborees of the Triple A, but also some regional societies, Physical Anthropology, Applied Anthropology, the occasional Archaeology meeting, even a few Sociology meetings and more New York Academy of Science meetings than I would care to remember. Each had their own particular quirks, but as a meeting maven this participant observer soon came to feel like a pro-. It might be useful to look at a number of predictable variables. First, that the national economy is reflected in the meeting behavior. When jobs were scarce (1930's 1940's and again since 1975) high jinks are at a lower ebb than in times of affluence, tension among the job hunters runs high and behavior reflective of the play of the principle of limited good comes to the fore (a principle not yet invented in the 30's and 40's, but present as a phenomena of the economic system nonetheless) seen in secretiveness and protection of sources of information. During times of expansion and affluence (particularly marked by the post World War II involvement of the Federal Government in the funding both of Research and of Graduate Training) there was open competition for new students seen not only in polished, PR type brochures, but also in the departmental parties given at the national meetings, where department after department not only started up new graduate programs, but also tried to outdo each other in the size and liquor flow at their social gatherings.

Students rated these "shindigs" not only by how many and which guests turned up, but by how far down the corridor the party spilled and how often the hotel dicks came by to shut down the noise. The truly successful parties were raided by the local police force. One also rated the meetings, during this period, by the extent of the publishers parties and by how many one could crash. Indeed this participant observer noted some strange ethical (?) shinanigans at some of these gatherings - as free loaders swilled from party to party and where, at one such gathering, a group of self-assured graduate students loudly proclaimed that they didn't care about the missions of the funding governmental agencies - they were just going to take the money and run.

By the early 1960's meeting size was clearly getting out of hand and the executive board decreed that it was going to face the problem of Leviathan by sounding the members to develop a scheme to change the structure of the meetings. It is in this setting that the faith of a true believer in the practice of the social scientist came to the fore - in the mistaken notion that by approaching the question in a scholarly fashion it would be possible to discover what the meeting meant to those participating in it, so that change could proceed in a rational fashion. The findings of this quantitative study summarized below represent those results where X^2 distributions were significant at, or below, the .01 level. About 15 students aided in unobtrusive auditory sampling (presently called "lurking") as well as in participant observation. The study documented much of what we already knew instinctually. First, that people like things just as they are - so long as there is room to bitch. In addition it should surprise no-one to learn that our status of American first (bound and tied to the cultural "givens") and social scientist second comes through loudly and clearly. We operate with a social system highly age and status graded and even the world of the convention-goer reflects this penetration. We wrote then, and would conclude the same today, that "despite expressed concern over the large number of non-professionals (or "outsiders") on the program, the assistant and associate professors are significantly over represented, while

students are consistently under-represented. Students, however, do not consider being on the program of prime importance, but for others (fully professionalized) this is either a primary or secondary consideration in their decision to come to the meeting. Being on the program is not only a factor in the decision to attend, but the disgruntled, whose submitted papers - or sessions - had been rejected were clear in stating that non-inclusion motivated them to stay home." Post World War II government largesse clearly set the stage for this phenomena, following which the notion of paying one's own way to a meeting came to be considered infra-dig. This has, obviously, led to the sort of multi-session paper free-for-all with which we are all too familiar.

Women were less inclined to rate paper presentation highly. Aha! say the clever ones, this should lend support to those who say that women are less productive than men. Not so - for one has to look to the distribution of women within the sample as a whole -and here we find that the great preponderance of women in those samples were students, and relatively few were fully professional - thus, what we have, is a reflection of the over-all low status of women rather than a lack of productivity.

What about listening to these marvelous papers which one is (sometimes) subsidized to present? Once again, we see age grading and status coming into play. Only students consistently rated attendance at paper sessions as of primary significance to their involvement with the meetings. For all other professional ranks (and rank and age are directly correlated) we find that this is a secondary reason for attendance. Thus, overall, the Ph.D.'s rated high as dispensers of data, those with lesser degrees were the consumers. Those who attend only once over 2-3 years were more involved in listening to papers (to catch up on a lack of reading?) than the regulars. Thus we create (through the social system) the situation where complaints are generated about the lack of intellectual controversy - since the highest statused, most active members don't go to hear (but only to give) papers, while the auditors are somewhat marginal to the active centers of research: students, neophytes and women, who would rather listen than talk.

How do networks get established? With whom does one interact at meetings? Clearly, with friends or colleagues of long standing, with those sharing one's own research interests, with people one wishes to influence in one way or another, and with those one meets for the first time. The youngest participants not yet having "old friends" downgrade this aspect of meeting activity, and it is the older Ph.D.'s who jam the corridors and bars, and stand on chairs to peer over to the other side of the room to find colleagues now living in widely distant parts of the country. Only the neophytes see the meeting as an important place to meet new people. Establishing oneself within the group is effected by forming ties with others. With advancing age (and presumably also advancing status) the prospect of meeting new anthropologists diminishes in attractiveness. So we have here the raw material for a situation where non-formalized, but operative, age-grades come into existence. The established Ph.D.'s spend their time seeking old, familiar faces and maintaining professional contacts, there is little time left for newcomers. The latter, being largely unsuccessful in their upward reaching, seek out similar neophytes from elsewhere in the country, have a (vicarious) satisfaction derived from indirect contact with greatness elsewhere, and begin to lay the groundwork for their own network of friends and colleagues with whom to interact in the years of their own greatness.

So much for the more formal aspects of convention behavior. What other observed changes can be documented, beyond the burgeoning size to which we'll return in a moment? My understanding of the history of the association is that it was during World War I that the decision was taken to start the move away from being a strictly scholarly society, in order to "be more effective" in Washington. Hindsight suggests that there was some desire here for anthropologists to "get a piece of the action". By the time of the second world war, government anthropologists already knew their way around and support for that effort was generally forthcoming. I must have slept through the Korean incident, for I don't remember much involvement of anthropologists at that time. But the Vietnam decade affected our profession as much as it did the rest of the country and, I would suggest, in

very parallel fashion. We were politicized as never before, and the schisms within the profession may never be fully resolved. Again, as the "disinterested" observer it might be worth noting that an inflexible organization which didn't permit non fellows to attend the council meetings (and certainly not to participate in the discussions) helped establish the schism. Since the non-fellows couldn't participate they decided to hold their own meetings focusing on their own concerns - and from this Association of Radical Political Anthropology was born. This was also the period of institutionalizing visible stratification - as first fellows and members were given different name tags (carefully marked M and F - which led some of us with a different code for these letters to wonder if the association really didn't know the difference between the sexes). This was followed by carefully color coded nametags (to sort out the racially yellow fellows from the racially green members, and both from those inbetween who were neither members, nor fellows, but merely students).The more recent present has been marked by further fragmentation of the association into both interest groups (fissioning off of special interest segments: medical, educational, applied, economic, political, legal, etc.etc.etc.) and pressure groups (the latter reflecting the concerns of the wider society - for affirmative action with regard to ethnic and sex groups, for jobs, for environmental actions, for pure air (anti-smoking), for stands on all sorts of issues). The disinterested observer has been struck by the ventilation effect of much of the activism as contrasted with the lesser energy given to the follow-through, i.e., there seems to be as much satisfaction taken from the "we told 'em" syndrome and relatively little follow through to those agencies where information on "stands" might have some effective leverage.

And so, now that we are fully expert in the lore and wisdom of conventioneering what general conclusions can we draw. Perhaps most important is that we are not immune - we are as trapped in our cultural constraints and contexts as are the Inuit, the San, the Yanomamo or the English. Being convinced social scientists doesn't lessen the pain of recognition when, with well-meaning naivte, we come to realize that "we have met the enemy, and he is us". Not science, not even social science, has the key to solve mankind's problems - perhaps only the keyhole through which we may spot some of the available solutions. So just like

the rest of the society we are culturally trapped; just like the rest of the society we're age and status-graded, and finally - a bit more shocking - we're dreadfully conservative - not only in terms of wanting to shield the people we study from the pains and blows of industrial society, but also in terms of not wanting to jostle our own comfortable social structure either. In short, neither studying others nor ourselves provides the necessary wisdom or strength to learn from their mistakes or even from their solutions. So, if we don't benefit from what we've learned about society either from self-study or from the investigation of the solutions of other people, where does this leave us? We seem to be in a position (sometimes) to predict and plan for others, but not for ourselves. "We know what's good for you", but please don't ask us to take the same medicine.

Now, to return briefly to the points made at first. This long term self-study was made as an act of faith - because this (triple A) was the society I then believed in. I thought, at the inception, that "it" and ego shared the same (or similar) ethos, and so the system would be both understandable and manageable. What was found following the idea of the replicate social structure, was that this "society" is only a small microcosm of the larger one "out there" - sharing all the disjunctions, pressures and problems of the larger society, and reflecting a similarity of responses to the positive excitements of being part of both the small and the whole. So further replication aside, how is one to "misread" the interpretation to get to a newer groundplan? Very tentatively and very hypothetically to suggest that all we've been observing, whenever, has been a fancy set of feathers adorning a very plain biped who, once stripped, only makes a limited number of maneuvers. The actions (maneuvers) are tied to the number of interactions possible at any one place and time; and the variety of responses to be anticipated is more a function of size, than of particular sociocultural constellation. Thus, crowding behavior in all socially structured situations will elicit similar responses and if so - it may be fruitful - for a while - and focus on the par-human similarities rather than on the multi~~si~~mplicity of diverse variables. There is, for the moment, no telling what new insights such a rereading may effect.